THE

QUARTERLY JOURNAL

OF

ECONOMICS

NOVEMBER, 1908

THE STATISTICAL COMPLEMENT OF PURE ECONOMICS.

SUMMARY.

Representative constructive workers in pure economics have conceived of an inductive statistical complement without whose development the *a priori* instrument must lack concrete effectiveness, 1.—The most important statistical processes that must be employed in the elaboration of the inductive science are described, 8.—Steps which have already been taken in the attempt to bring together pure economics and the theory of statistics in the treatment of concrete material are illustrated, 23.—From a survey of what has been accomplished it is inferred that fecund scientific ideas as well as abiding practical results are to be gained from the development of the *Statistical Complement of Pure Economics*, 33

In the case of every science it is wise economy to make an appraisement, from time to time, of dominant conceptions and prevailing methods. Like the relative values of material commodities the relative values of ideas and processes shift with the passing of time, and the appearance of new problems amid the pressing exigencies of practical life requires that we frequently take stock of our scientific capital, if the expenditure of energy is to yield its proper return. The economist particularly should

attend to the changing efficiency of his scientific machinery, for he himself has taught that the value of capital is proportional to its yield, and that the maximum yield is in the direction of greatest social service.

Of the investigators who, since Ricardo's day, have contributed most to the development of scientific economics, there are four [1] whom the consensus of the opinions of their colleagues would certainly not place below the first rank: Cournot, Jevons, Edgeworth, and Pareto. All four have contributed a very large part of their work to the elaboration of the deductive phase of our science, but each of them has conceived of an inductive statistical complement of the pure science without whose development the *a priori* instrument must lack concrete effectiveness. They have all proceeded on the assumption that the greatest need, at the time of their writing, was a correct theory of economics, in order to afford a first approximation to reality and to show what is required to solve concrete problems. But they have likewise made fundamental contributions to the inductive statistical complement of the pure science.

It is the object of this paper:—

I. To show that these representative constructive workers in the elaboration of pure economics have had in mind an inductive statistical complementary science;

II. To describe and illustrate the fundamental statistical processes that must be employed in the inductive science;

III. To indicate the manner in which the theory of economics and the science of statistics are being brought together in the development of the *Statistical Complement of Pure Economics.*

[1] These four economists have been named because they conspicuously illustrate the points I wish to make. Within the limits of economists of the first rank, their general views are, I think, representative.

I.

One of the curious facts in the history of Cournot's varied and always profound work is that, while one of the chief merits of his little treatise on the mathematical principles of wealth consists in his discovery and illustration of the power of mechanical and geometrical methods in the treatment of certain questions in economics, he himself has said that the most fertile instrument of inquiry will be found to be the statistical method:—

Si nous restons dans l'ordre des causes secondaires et des faits observables, le seul auquel la science puisse atteindre, la théorie mathématique du hasard . . . nous apparaît comme l'application la plus vaste de la science des nombres, et celle qui justifie le mieux l'adage: *Mundum regunt numeri.* En effet, quoiqu'en aient pensé certains philosophes, rien ne nous autorise à croire qu'on puisse rendre raison de tous les phénomènes avec les notions d'étendue, de temps, de mouvement, en un mot, avec les seules notions des grandeurs continues sur lesquelles portent les mesures et les calculs du géomètre. Les actes des êtres vivants, intelligents et moraux ne s'expliquent nullement, dans l'état de nos connaissances, et il y a de bonnes raisons de croire qu'ils ne s'expliqueront jamais par la mécanique et la géométrie. Ils ne tombent donc point, par le côté géométrique ou mécanique, dans le domaine des nombres, mais ils s'y retrouvent placés, en tant que les notions de combinaison et de chance, de cause et de hasard, sont supérieures, dans l'ordre des abstractions, à la géométrie et à la mécanique, et s'appliquent aux phénomènes de la nature vivante comme à ceux que produisent les forces qui sollicitent la matière inorganique; aux actes réfléchis des êtres libres, comme aux déterminations fatales de l'appétit et de l'instinct.[1]

Indeed, altho Cournot is known to economists almost entirely through his *Recherches*, he has made very great contributions towards the construction of an inductive, statistical science. His treatise *Exposition de la théorie*

[1] Essai sur les fondements de nos connaissances et sur les caractères de la critique philosophique, vol. i. pp. 64–65.

des chances et des probabilités, which was outlined at least two years[1] before the publication of his *Recherches*, and which, according to a competent critic,[2] is not excelled in philosophic acumen by the works of Laplace, had as its essential aim to show that the theory of probability is the foundation of all induction, the basis of statistical methods and the justification of the ancient adage, *Mundum regunt numeri.*

His very lofty conception of statistics is described in his chapter "De la statistique":—

> Pour que la statistique mérite le nom de science, elle ne doit pas consister simplement dans une compilation de faits et de chiffres: elle doit avoir sa théorie, ses règles, ses principes. Or cette théorie s'applique aux faits de l'ordre physique et naturel, comme à ceux de l'ordre social et politique. En ce sens, des phénomènes qui s'accomplissent dans les espaces célestes peuvent être soumis aux règles et aux investigations de la statistique, comme les agitations de l'atmosphère, les perturbations de l'économie animale, et comme les faits plus complexes encore qui naissent, dans l'état de société, du frottements des individus et des peuples.[3]

Jevons has generously conceded to Cournot priority in the general mathematical conception of economics and in the logarithmic method of treating index numbers, but his kinship with Cournot in point of view, philosophic temper and method of inquiry did not end with these two subjects. A very large part of Jevons' *Principles of Science* had been eloquently set forth in Cournot's *Essai sur les fondements de nos connaissances*, and in his *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire*. The key to the philosophic position of

[1] See the date of the letter from Poisson, printed on page vi of the Théorie des chances

[2] Czuber, Wahrscheinlichkeitsrechnung, p. 15.

[3] Exposition de la théorie des chances et des probabilités, p. 184.

both is in their effort to lay the foundation of all induction in the theory of probability.

Jevons himself has told us of his early respect for the statistical method and of his wish to excel in its use. After the publication and gratifying reception of his essays on "A Serious Fall in the Value of Gold Ascertained" and "The Coal Question," he wrote, "I distinctly remember thinking in Sydney that, if there were one thing I should wish to be, it would be a recognized statistical writer. How strangely my wish has been fulfilled!" [1]

The complementary character of inductive, statistical economics and pure economics he tersely describes in his Theory of Political Economy:—

> I know not when we shall have a perfect system of statistics, but the want of it is the only insuperable obstacle in the way of making Economics an exact science. In the absence of complete statistics, the science will not be less mathematical, though it will be immensely less useful than if it were, comparatively speaking, exact. A correct theory is the first step towards improvement, by showing what we need and what we might accomplish.
>
> The deductive science of Economics must be verified and rendered useful by the purely empirical science of statistics. Theory must be invested with the reality and life of fact. [2]

Like Cournot and Jevons, Professor Edgeworth was early interested in the methodology of inductive social sciences. Shortly after the publication of *Mathematical Psychics*, he began a series of studies in the Philosophical Magazine and in the Journal of the Statistical Society which seems to have had the threefold object: (1) of inquiring into the philosophic basis of the methods of induction used in the so-called exact sciences, notably of the Method of Least Squares and of the Law of Error; (2)

[1] Letters and Journal, p. 223.

[2] Third edition, pp. 12, 22. Cf. Principles of Science, chap xxii. End of the section on "Illustration of Empirical Quantitative Laws."

of exhibiting the utilitarian character of the ultimate foundation of inductive inference; (3) of fashioning out of the material of the methods of the natural sciences an inductive instrument fit for the use of the social sciences.

His creative activity in this direction has been continuous. Only very recently he has brought to a conclusion an investigation, dating as far back as 1898, the purpose of which is the development of a generalized method of representing statistical data by mathematical formulæ.[1] His principal thesis will, I am sure, fructify abundantly in the inductive stage of economic science which we are entering:—

> The representation of numerical observations by a mathematical formula is then most perfect when *à priori* it is demonstrable that the formula tends to correspond to the facts, and actually the correspondence proves to be close.
>
> In cases where the normal law is evidenced both by *à priori* reasoning and observed fitness, there can be no hesitation about preferring that law. The rub is where the law is no longer accurately fulfilled, as in the case of unsymmetrical groups of observations. *The main thesis of this paper is that preference should be given to formulæ which have a certain affinity to the normal law, which are regarded as due to some modification of the conditions under which the normal law arises.* Those conditions are mainly two: the co-operation of an indefinite number of independent agencies and the smallness of the effect of each agency on the total result. The existence of these conditions *in rerum natura* is probable prior to the observation that the normal law is of frequent occurrence. The prevalence of that law attests the prevalence of the conditions from which it is deducible. *It is a probable hypothesis that some modification of those conditions results in a somewhat abnormal law of frequency.*[2]

Without, at the time, knowing of Professor Edgeworth's general thesis, I gave, in my study on "The Efficiency

[1] Journal of the Royal Statistical Society, December, 1898; March, 1899, June, 1899; September, 1899, March, 1900; September, 1906.

[2] Journal of the Royal Statistical Society, December, 1898, pp. 671, 674. I have taken the liberty of italicizing the two sentences to which I wish particularly to draw attention

Theory of Wages," a particular illustration [1] of the advantage of treating skew curves in social statistics as having their origin in a disturbed condition of normal distribution. To this illustration I shall recur.

Since the publication in 1896–97 of the *Cours d'économie politique*, Professor Pareto has divided his time between deductive and inductive inquiries. He has devoted himself to investigations in two directions: (1) to a simplification and generalization of the mathematics of his pure economics, which appears in the appendix of his *Manuale di Economia Politica* (1906); and (2) to the development of methods of interpolation for use in inductive inquiries, which appears in *Tables pour faciliter l'application de la méthode des moindres carrés* (1898), and in the series of articles now in course of publication in the Giornale degli Economisti on "L' Interpolazione per la ricerca delle leggi economiche."

Perhaps it would be an exaggeration to speak of a Pareto school of inductive economics in Italy, but surely it is true to say that his inductive study of the distribution of income supplied instruments for the breaking of the new ground that has been cultivated with profit by Professor Benini and Dr. Bresciani.

It will be granted, I think, that Professor Pareto has reached the most general *a priori* treatment of the whole problem of production and distribution, and I therefore regard as of great value his opinion as to the most promising direction of investigation in economics.—

> The progress of political economy in the future will depend in great part upon the investigation of empirical laws, derived from statistics, which will then be compared with known theoretical laws, or will suggest derivation from them of new laws.[2]

[1] Economic Journal, December, 1907.

[2] "Il progresso dell' Economia politica dipenderà pel futuro in gran parte dalla ricerca di leggi empiriche, ricavate dalla statistica, e che si paragoneranno poi colle leggi teoriche note, o che ne faranno conoscere di nuove Quelle leggi empiriche sono in sostanza date dall' interpolazione dei dati statistici, onde da ciò appare la grande importanza di tale operazione" Giornale degli Economisti, Maggio, 1907, p 366.

II.

Pure economics and the theory of statistics have passed through similar phases in their assumption of scientific form. A characteristic of pure economics has been the progressive subjugation of outlying territories by means of the calculus of value: a characteristic of theoretical statistics has been a similar conquest by means of the calculus of probability. The calculus of value and the calculus of probability have been the two engines which have extended the dominion of social science.

Some years ago, in his address on "The Present Position of Economics," Professor Marshall observed, in referring to Ricardo and his followers, that "they did not make their drift obvious. They did not make it clear to others, it was not even quite clear to themselves, that what they were building up was not universal truth, but machinery of universal application in the discovery of a certain class of truths" (p. 19). . . . "The true *raison d'être* of theory is that it supplies a machinery to aid us in reasoning about those motives of human action which are measurable." (p. 22.)[1]

Similarly, the theory of probability as applied to the social sciences is not a body of concrete doctrine, but rather a machinery of general application in the study of the mass-phenomena upon which the social sciences rest.

Particular directions in which the theory of probability has yielded results where other instruments have proved inadequate are in

(1) The evaluation of the most probable amid contingent values;

[1] I do not stop to exploit the fact that in later years Professor Marshall developed the doctrine that the measurable motives with which the economist is concerned are investigated only in so far as they are manifested in the action of large groups, and that the pure science of economics, therefore, postulates the mass-phenomena which it is particularly the province of statistics to describe. Principles of Economics, 4th edition, Book I, chap v

(2) The definition and discovery of the type amid variety;

(3) The derivation of the continuously variable from the discontinuously variable.

These three processes put the investigator in the way of resolving complex phenomena into the relations of their constituent elements. The first two processes I shall deal with only to the extent of briefly describing and illustrating them. The third shall be given greater attention, in as much as its importance has not received, until very recently, the emphasis which has been given to the other two.

The evaluation of the most probable amid contingent values is essentially the problem of determining the reliability of a mean and the reliability of functions of means.

The problem may be illustrated by considering this question: Suppose it is discovered that in a given population the average stature of 1,000 wives is less by x inches than the average stature of 1,000 female adults selected at random. Is the difference in the averages significant of the presence of a true cause—sexual selection or child-bearing—or is the difference attributable to chance? In what way would the importance of the difference in the averages be affected by the numbers of the individuals in the groups of which the averages were taken? This is a problem of the significance of the difference of two means, which is the simplest form of problem dependent upon the reliability of functions of means.

To arrive at *verae causae*, we must eliminate chance, and, to accomplish this end, we make use of the theory of the law of error.

Suppose we define an error as the difference between

any particular measurement, in a group of measurements, and the average measurement of the group. The law of error, then, is the law of the distribution of the series of errors about a zero point. The distribution will take the form described by the curve $\frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{x^2}{2\sigma^2}}$, where the origin is at zero, σ is the standard deviation of the measurements, and x is any particular error. The probability of an error between x and $(x+dx)$ is $\frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{x^2}{2\sigma^2}} dx$.

The "probable error" in the series of errors is that deviation either side of the origin between which and the origin 25 per cent of the errors fall. To determine the reliability of an average, it is necessary to determine the probable error of the average, and wherever functions of averages are dealt with we must determine the probable error of the functions. In case of the question just asked concerning the significance of the difference in the average stature of wives and the average stature of adult females, it is necessary to determine the probable error of the difference between the two averages.

For the solution of problems of the reliability of means and of functions of means, the most important formulæ derived by the Method of Least Squares are:

(1) The probable error of an average is $\frac{.6745\sigma}{\sqrt{n}}$, where σ is the standard deviation and equal to $\sqrt{\frac{\Sigma x^2}{n}}$, Σx^2 being the sum of the squares of the errors and n the number of the observations.

(2) The probable error of functions:—

Let $m_1, m_2, \ldots m_n$, be a series of independent quantities

whose probable errors are respectively $r_1, r_2, \ldots r_n$. Then the probable error of $F(m_1, m_2, \ldots m_n)$ is

$$\sqrt{\left(\frac{\partial F}{\partial m_1}\right)^2 r_1^2 + \left(\frac{\partial F}{\partial m_2}\right)^2 r_2^2 + \ldots \left(\frac{\partial F}{\partial m_n}\right)^2 r_n^2}.$$

In the particular case of the probable error of the difference between two means, we find by using formula (2) that the probable error of the difference is equal to the square root of the sum of the squares of the probable errors of the means. By employing the law of distribution of errors, it is easily possible to determine exactly the probability of an error of any particular magnitude. Consequently, the probability that an error should be any multiple of the probable error is equally easily determined. For example, the probability that an error should exceed twice the probable error is .1773; that it should exceed three times the probable error is .043; four times the probable error, .007. If, therefore, an observed difference between two means exceeds the probable error of the difference in any given ratio, it is possible to determine exactly the probability that the difference is due to chance. The smaller the probability that the difference is due to chance, the greater the probability that it has its origin in a *vera causa*.

Equipped with this simple instrument for determining the probable error of the differences between means, the investigator may at once ascertain the truth or folly of many theories. For example, let us turn to our question as to whether sexual selection with respect to stature occurs in modern societies. Professor Pearson[1] found, in a particular case, that the average stature of wives was 63.869 inches with a probable error of .110; and the average stature of females in general was 64.043 with a probable error of .061. The difference between the

[1] Grammar of Science, pp 426–427

average stature of wives and the average stature of adult females is $(63.869 - 64.043) = -.174$. The probable error of this difference is $\sqrt{(.110)^2 + (.061)^2} = .126$, which is nearly equal to the difference itself, and, consequently, Professor Pearson argues that, in this case, the difference between the two averages cannot be said to be significant.

Professor Edgeworth's "Methods of Statistics"[1] abounds in illustrations of the utility of this simple formula. As one of his illustrations, the following may be cited: In the final report of the Anthropometric Committee of the British Association, 1883, the average height of 2,315 male criminals was found to differ by two inches from the average height of 8,585 members of the adult male population. Professor Edgeworth queries whether the difference of two inches is significant of the presence of a cause or is merely accidental. The formula for the probable error of the difference of two averages gives the solution of the problem. The probable error of the average stature of criminals was .036, and the probable error of the average stature of the general male population was .018. The probable error of the difference between the two averages was, therefore, $\sqrt{(.036)^2 + (\ 018)^2} = .04$. This probable error is so small, when compared with the observed difference of two inches, that we are certain of the presence of a true cause. We know from the law of error that, when the probable error of the difference of two averages is .04, the probability of a difference of 2 occurring by chance is infinitely small.

The problem of the description and discovery of the type amid variety has led to two lines of inquiry connecting themselves, respectively, with the names of Galton and Pearson, and Lexis and Von Bortkewitsch.

The first inquiry is no less than an endeavor to demon-

[1] Jubilee Volume of the Statistical Society, 1885

strate Darwinism by means of statistical methods.[1] The terms used by Darwin admit of such quantitative definition that statistical studies may be planned to test evolutionary hypotheses. The theory and technique employed in these investigations are doubtless the chief development of the theory of probability and its application since the work of Laplace. It is not intended, however, to describe here what, in general outline, is already familiar to students of *Natural Inheritance* and of the *Grammar of Science*. Economists and sociologists who will have need to employ more and more the generalizations of the science of evolution may perhaps be reminded of Professor Pearson's methodological warning:[2]—

> To the writers who talk of this result or that being due "to the relative variability of local races," who assert that a peculiarity is "a result of the correlation of two organs," or who attribute this or that change of character to heredity, to reversion or to telegony, we now simply say: What is the numerical value of the variability of which you speak? Have you a measure of this correlation? Did you test the magnitude of the inheritance of that character? What is the nature of the inheritance in the case of the character which you attribute to reversion or to telegony? Till very definite answers are forthcoming to these questions, we are not in the present state of our knowledge bound to pay much attention to those who are over ready to "explain" not only organic, but social changes by a vague use of undefined biological terms.

The problem which Professor Lexis[3] and Professor von Bortkewitsch[4] have set themselves to solve is this: Under what conditions may statistical ratios be assumed to be empirical functions of a probability which is determined by the nature of things and which either

[1] Biometrika, vol. v pp. 17, 26

[2] Grammar of Science, p. 372.

[3] "Geschlechtsverhaltnis der Geborenen und Gestorbenen." In the Handworterbuch der Staatswissenschaften. Also, Zur Theorie der Massenerscheinungen, and Theorie der Bevolkerungs- und Moralstatistik.

[4] Das Gesetz der kleinen Zahlen.

remains constant or varies according to law? For example, it has been observed to be a general fact that the ratio of the number of males born to the number of females born exceeds unity, and that, within a limited time and place, the ratios of males and females born fluctuate about a constant value. Is it legitimate to infer that the constant average value is predetermined by the nature of things, and that the several ratios are variations from the average effected by accidental causes? The inquiry is of the greatest importance, for it is an endeavor, not only to fix the general facts of human society, but also to determine those facts which are purely accidental.

The leading steps in Professor Lexis' argument are these: Suppose n groups of g drawings are made from an urn containing an unlimited number of white and black balls in a definite ratio. Suppose, further, that the number of white balls drawn in the n groups are respectively $m_1, m_2, \ldots m_n$. The following results will be found to be true:—

(1) The several ratios $\frac{m_1}{g}, \frac{m_2}{g}, \ldots \frac{m_n}{g}$ will arrange themselves around their mean $\frac{m_1 + m_2 + \ldots m_n}{ng.} = v$, according to the law of error;

(2) The "precision" of the distribution, which we shall call h, will be $\sqrt{\frac{g}{2v(1-v)}}$. The greater the value of h, the more closely will the several ratios cluster about their mean; and the smaller the value of h, the more widely will the ratios be scattered.

The "precision" of a distribution determined in this manner, which is the same as the method employed in games of chance, Professor Lexis calls the "combinational precision." His argument as to whether a particular

distribution of ratios is analogous to a chance distribution hinges upon the possibility of determining the precision of distribution in another way. If the respective differences between $\frac{m_1}{g}, \frac{m_2}{g}, \ldots \frac{m_n}{g}$ and their mean v be computed, then, by the Method of Least Squares, the precision of the distribution is $\sqrt{\frac{n-1}{2\Sigma x^2}}$, where x is the difference between any particular ratio and the mean of the ratios. The precision of distribution determined in this manner is called by Professor Lexis the "physical precision."

If it be found that a series of statistical ratios is distributed according to the law of error, and that the combinational precision is equal to the physical precision, then, according to Professor Lexis, the series is a *typical series* with *normal dispersion.* The deviations of the items of the series from their average may be regarded as accidental. If the distribution is according to the normal law and the physical precision is greater than the combinational precision, the series is a typical series with *infranormal* (*unternormaler*) *dispersion;* that is, the ratios cluster very closely about the mean, indicating a connection between the conditions of the successive ratios. If the distribution is according to the normal law and the physical precision is less than the combinational precision, the series is a *typical series* with *supernormal* (*ubernormaler*) *dispersion;* that is, the ratios are greatly scattered about the mean, indicating, a change in the condition of the underlying probability of the series.

If the distribution does not follow the law of error, it is called by Professor Lexis a *symptomatic series.* These symptomatic series are further classified into groups, the most important of which are the periodic series and the evolutionary series, the latter indicating a progressive

change in the fundamental conditions of the phenomena under observation.

Professor von Bortkewitsch has made a special study of the Lexis theory in cases where the statistical ratios are very small numbers; *e.g.*, the number of suicides among women in small German provinces.

The problem of the derivation of the continuously variable from the discontinuously variable is essentially the problem of interpolation. In the development of this topic I should like to take as the point of departure the discussion of the problem of method and truth which has become familiar to us through the eloquence of Professor William James. My particular reason for referring to the pragmatic doctrine on this subject is to urge that the problems of truth and method which the pragmatists are endeavoring to handle are the same problems with which mathematical statisticians have been concerned; and that, however remote [1] the theory of statistics may appear from pragmatic philosophy, the statistical method of interpolation exemplifies the doctrines which are faintly adumbrated in the teaching of the new school.

In his essay Truth,[2] Dr. F. C. S. Schiller sets himself the task of explaining how pragmatism extends and alters the traditional conception of truth. His own positive conclusions may be summarized in these propositions:—

(1) Truth is a form of social valuation. "Truth, then, to be really safe, has to be more than individual valuation; it has to win social recognition, to transform itself into a common property. But how? It is by answering this

[1] The subjects are by no means so far apart as one would at first suppose A cofounder of the pragmatic school, Professor John Dewey, has said "Laws are not governmental regulations which limit change, but are convenient formulations of selected portions of change followed through a longer or shorter period of time, and then registered in *those statistical forms which are amenable to mathematical manipulation.*" Lecture on Ethics, New York, 1908.

[2] Humanism Philosophical Essays, pp. 44–61.

question that Pragmatism claims to have made a real advance in our comprehension of truth. It contends . . . it is the usefulness and efficiency of the propositions for which 'truth' is claimed that determines their social recognition." "Truth is the useful, efficient, workable, to which our practical experience tends to restrict our truth-valuations; if anything the reverse of this professes to be true, it is (sooner or later) detected and rejected."

(2) The grounds of choice between rival truths are:

(*a*) Their relative utility: "Whenever we observe a struggle between two rival theories of events we find that it is ultimately the greater conduciveness of the victor to our use and convenience that determines our preference and its consequent acceptance as true."[1]

(*b*) Their relative simplicity: "When I affirm . . . that the metaphysical theory of the absolute is *false,* I only mean that it is useless, that it simplifies nothing and complicates everything."[2]

It has frequently been observed that a scientific theory, which is intended to summarize a number of facts of observation, may be likened to a curve that is drawn subject to the condition of passing through definite points. As it is possible, mathematically, to draw an indefinite number of curves through given points, so likewise it is possible to offer an indefinite number of theories in description of given facts of observation. The selection of the appropriate theory—which we call the true theory—raises the same questions as the interpolation of the appropriate curve. An examination of the grounds of the procedure in the interpolation of the appropriate curve will therefore throw light upon the problem of the choice of a true theory. I shall aim to show that utilitarian

[1] Humanism: Philosophical Essays, pp 58–59. [2] Ibid., p 59.

considerations determine the character of every step in the process of interpolation. Besides illustrating the authority of the principle of utility in all scientific work, a review of the principles of interpolation will serve the purpose of describing one of the most valuable instruments employed in the inductive treatment of economics.

Suppose that, with given co-ordinates, a series of points is plotted upon a surface and it is required to give a mathematical definition of a curve that will summarize the points. The two principal questions that present themselves are these:—

(1) What type of curve shall be selected?

(2) What shall be the criterion of the fit of the curve to the points?

In the selection of the type of the curve the principal grounds of choice, so far as I am aware, have been as follows: (*a*) the simplicity of the curve; (*b*) its fecundity; (*c*) its fit; (*d*) its facility of computation; (*e*) its *a priori* validity.

(*a*) The simplicity of the curve. The impossibility of rigidly defining what is simple and what is complex has not escaped statisticians. It is quite possible that from a particular point of view the equation of one type of curve might be more simple than that of another type, and yet be more complex when viewed in another light. It might, for example, have fewer constants, and, from this point of view, be more simple than another equation with a greater number of parameters. But the evaluation of the constants in the first case might entail an extremely complex operation, while, in the latter case, no difficulty would be encountered. An inflexible definition of the simple is, therefore, not offered, but it may be said that, in statistics, *simplicity is relative to the state of analysis and to the practical end in view.*

As an illustration of a statistician's reasoning, Mr. Yule's explanation of his use of Tait's formula may be instanced. Tait's formula is a description of the law of the variation with age of fertility in women. Upon the basis of the data for Scotland in 1855, Professor Tait discovered that if f were put equal to the percentage of wives of age t who in course of the year became mothers, then $f = k\ (50-t)$; or the percentage of wives who in course of the year became mothers varied directly with the difference between 50 and the age of the wives. In the case of the Scottish figures for 1855, k was equal to 1.5.

Now observe the purely utilitarian—or pragmatic—nature of Mr. Yule's argument[1] in his grounds for the use of Tait's law. He regards Tait's law as "an extremely simple law."[2] And, while it is only "a rough empirical formula," nevertheless, "for the purposes of weighting—the practical end Mr. Yule has in view—a very exact law is not essential." "Further, the simple linear law has two great advantages: (1) it permits of the immediate calculation of the relative fertilities of groups of any age-limits without the necessity of interpolating between observed results, a very useful property where different census returns give different groupings, and (2) it introduces a 'coefficient of fertility' with a direct and simple physical meaning."[3]

Here we have the choice of a simple empirical formula because of its analytical and practical advantages.

(*b*) The fecundity of the curve. As an illustration, Gompertz's formula for the value of l_x in a mortality table may be given; namely, $l_x = kg^{c^x}$, where x is the age of the group, l_x is the number who have attained the age, and k, g, c, are constants to be determined from the

[1] "On the Changes in the Marriage and Birth Rates in England and Wales during the Past Half-century, with an Inquiry as to their Probable Causes" Journal of the Royal Statistical Society, March, 1906.

[2] Ibid, p. 115. [3] Ibid, p. 116.

special data. The utilitarian reasons for the choice of this type of curve may also be noted:—

"... for the purpose of the actuary a dominant circumstance in favour of Gompertz's law is that it lends itself to problems of *joint survivorship*. The formula of Gompertz, and *this formula alone*, enables us to calculate the value of a joint life annuity from a table of the values of annuities on single lives, by finding a single life which may be substituted for the two or more joint lives."[1]

(*c*) The fit of the curve to the data. Professor Pareto's formula for the distribution of income illustrates this point. If N be regarded as the number of persons receiving an income above the value of x, then, according to Pareto's law, $N = \frac{A}{(x+b)^{\alpha}}$ or $\log N = \log A - \alpha \log (x+b)$, where A, b, and α are determined from the statistics of income. The controversy between Professor Pareto and Professor Edgeworth seemed to turn upon the question as to whether excellence of fit is a sufficient reason for regarding a particular curve as the curve appropriate to the data.

That the question of the fit of a curve is a question of utility will be made clear in the discussion below of the criterion of fit.

(*d*) The facility with which the constants of the curve may be computed. Interpolation is always a laborious process, and consequently the existence of tables for facilitating computation according to a particular method of interpolation insures the continued use of that method. For example, the tables which have been computed for the aid of statisticians using Professor Pearson's method of moments will necessarily extend the use of the method.

[1] Professor Edgeworth's article "On the Representation of Statistics by Mathematical Formulæ," Journal of the Royal Statistical Society, December, 1898, p. 672. I have taken the liberty of italicizing the words "and this form alone." My debt to Professor Edgeworth, here and elsewhere, is very great.

Similarly, for the particular case of statistical data arranged according to an equi-crescent variable, the statistician will with advantage resort to Professor Pareto's table for facilitating interpolation by the Method of Least Squares.

(*e*) The "*a priori* validity" of the curve. This is Professor Edgeworth's phrase as well as his idea. According to the theory of Professor Edgeworth that type of curve is to be preferred for which there is an *a priori* demonstration that it will tend to correspond to the data, and which actually does correspond very closely. His preference for facility curves which are translated forms of the normal Gaussian curves is based upon this reasoning. In the third section of this paper I shall offer an illustration of the theory.

So far as Professor Edgeworth's reasoning is based upon the fit of the curve, it is utilitarian to the same extent as (*c*). The significance of "*a priori* validity" I take to be the utilitarian doctrine that hypotheses are not to be wasted. *Entia non sunt multiplicanda praeter necessitatem.*

The answer to our first question with regard to interpolation—namely, as to grounds for the choice of a type of formula—has now been reached. The considerations which have usually guided the selection have been the simplicity of the formula, its fecundity, its closeness of fit, its ease of calculation, its *a priori* validity. All five of these considerations rest ultimately upon a utilitarian basis.

We come now to our next question: After the choice of the type of curve has been made, what shall be the criterion of the fit of the curve to the observations? One judges of the excellence of a theory by its general accordance with the facts, and one judges of the excellence of the fit of a curve by its general accordance with observations. An attempt to give precision to the meaning

of *general accordance* will bring out the utilitarian character of our latent criteria.

If the interpolated curve is not to pass through all of the plotted points, what condition shall be fulfilled by the errors, or deviations of the points from the curve? Obviously, the possible conditions are infinite, and the choice of a particular condition has, in most cases, been made upon utilitarian grounds. Two illustrations may be given.

(1) The Method of Least Squares. This is the method most frequently used in the natural sciences, where, if anywhere, it would be supposed that utilitarian philosophy played no part. Nevertheless, "in the Method of Least Squares, as in the moral sciences, we are concerned with a psychical quantity—the greatest possible quantity of advantage."[1] The assumptions posited by Laplace in the demonstration of his theory were made because they were "advantageous in respect of convenience to the calculator and avoidance of trouble."[2] And the aim both of Laplace and of Gauss was to arrive at a solution which would minimize "the detriment incident to the errors which must in the long run be incurred."[3]

(2) The method of Cauchy. Recalling the fact that, from a purely mathematical point of view, a problem is adequately solved when the number of independent equations is equal to the number of unknown quantities, Professor Pareto remarks that the length of time required to evaluate the unknown quantities from the given equations may render the mathematical solution practically nugatory. His preference for the method of Cauchy over the usual Method of Least Squares, even when the degree of approximation attained is not so great, is based

[1] Professor Edgeworth's article on "The Method of Least Squares," in the Philosophical Magazine, November, 1883, p. 361.

[2] Ibid., p. 361.

[3] Professor Edgeworth's Metretike, p. 56.

upon the greater facility in calculation. "Il metodo di Cauchy è convenientissimo per la facilità dei calcoli, ma non è sempre ottimo per l' approssimazione." "Per le ricerche statistiche, si può sempre preferire, almeno per una prima approssimazione, il metodo di Cauchy; ed usare solo in casi speciali del metodo dei minimi quadrati." [1]

III.

During the greater part of the century there has been a parallel development of the machinery of pure economics and the machinery of pure statistics. Attempts in the direction of utilizing the two for the purpose of treating concrete material have, however, not been lacking. Some of these essays in scientific realism I shall recall.

At one time the most hopeful field of exact inquiry seemed to be in the investigation of curves of demand and supply, and, notwithstanding the small yield to the great labor that has been expended in this field, it is at present cultivated with renewed enthusiasm.

Jevons' treatment of the law of demand for corn according to the figures generally attributed to Gregory King seemed to promise an immediate connection of economic theory with arithmetic results. That his figures were nearly two centuries old was sufficient reason for his not carrying his results to a practical conclusion, and he contented himself with observing: "I know nothing more strange and discreditable to statists and economists than that in so important a point as the relation of price and supply of the main article of food, we owe our most accurate estimate to writers who lived from one to two centuries ago." [2]

Perhaps the next important stage in the development

[1] Giornale degli Economisti, May, 1907, "L' Interpolazione per la ricerca delle leggi economiche," pp 379, 385

[2] Theory of Political Economy, p. 154.

of this phase of the question was Professor Marshall's suggestion (1) that, for practical purposes, there is no need to know the curve throughout its whole extent, as seemed to be implied in Jevons' reasoning; (2) that points on the demand curve might be obtained by considering the demand of groups differently circumstanced in the matter of income.[1]

Recently the problem has been approached from two new directions.

In 1895, after Professor Pareto had discovered a method of representing briefly the distribution of wealth in a given community, he made a preliminary investigation[2] of the question as to whether the demand for a commodity is not a function of the form of distribution of wealth. With regard to a necessity of life he observed (1) that, in case of the very rich, the demand is comparatively constant, whatever the price; and (2) that, in case of the poor, the demand is very inelastic. Assuming that the individual's demand for a necessity of life is a function of his income, he was able to derive the total demand from the individual demands, and to show in what way it would vary with the nature of the distribution of income.

So late as the present year the subject has come to the fore. Upon the occasion of the meeting of the Italian economists at Parma in 1907, Professor Benini,[3] stimulated by the fact that the economists were holding their session as one section of a general association of physical and natural scientists, urged that economists should adopt in their investigations the methods of research employed by the natural scientists. In particular, he advised the use of methods of interpolation for the dis-

[1] Principles of Economics, 4th edition, Book III., chap. iv., particularly p. 189.

[2] "La legge della domanda" Giornale degli Economisti, January, 1895

[3] "Sull' uso delle formule empiriche nell' economia applicata." Giornale degli Economisti, November, 1907.

covery of empirical laws from statistical data, and instanced the utility of the evaluation of laws of demand and supply. In his inaugural address[1] as Professor of Statistics at Rome, he returned to the subject, proposing that the equations to the demand curves of the most important commodities be determined, their elasticities of demand be calculated, and the commodities be classified according to their respective elasticities. Professor Benini will attempt to carry out his programme in his academic work at Rome.

Perhaps the best-known empirical law of economics is Professor Pareto's law of the distribution of income, to which reference has already been made. If the incomes of a large community are tabulated in such a way that N represents the number of individuals having an income above x, then the curve $N = \frac{A}{x^{\alpha}}$ is a type of curve that will fit the statistics, A and α being two constants that are determined from the data by methods of interpolation. The equation to the curve may be written in the form $\log N = \log A - \alpha \log x$, which is an equation to a straight line. From this fact it follows that, if the logarithms of the successive rates of wages and the logarithms of the corresponding values of N be computed and plotted, the series of points will lie in a straight line. Furthermore, the value of α, which is the tangent of the angle made by the line with the axis of x, is an index of the degree of inequality of the distribution of wealth. Professor Pareto has preferred to regard the degrees of inequality of distribution as varying directly with the value of α, that is, the smaller the value of α, the less the inequality in distribution and *vice versa*. His critics have

[1] "Una possibile creazione del metodo statistico (L' Economia politica induttiva)." Giornale degli Economisti, January, 1908.

admitted his right to define the sense in which he proposes to use his terms, but have indicated that the opposite interpretation of α—namely, that the smaller the value of α, the greater the inequality in the distribution of wealth —is more in harmony with the mathematical theory of evolution and with common usage. I shall indicate some directions in which Professor Pareto's method may be employed.

Professor Bresciani,[1] utilizing Prussian figures of the distribution of income in cities, has shown that the value of α varies inversely with the size of the urban population. For example, in 1902, for cities of population between 50,000 and 100,000, the value of α was 1.56; for cities of population between 100,000 and 200,000 the value of α was 1.38; and for cities over 200,000 the value of α was 1.30.

Professor Adolph Wagner's [2] laborious investigations as to the trend of the distribution of income in Prussia offer invaluable material for the application of Pareto's method. Taking Ostpreussen as the extreme type of an agricultural province and Rheinland as the extreme type of an industrial province, Professor Wagner has tabulated the statistics of income in the two provinces during recent years. The questions which at once occur are whether there is a difference in the manner of the distribution of wealth as one passes from an agricultural province to an industrial province, and what is the trend of the variation of distribution with the flow of time. In the annexed tables, I have used Professor Wagner's figures [3] only for the incomes above 3,000 marks.

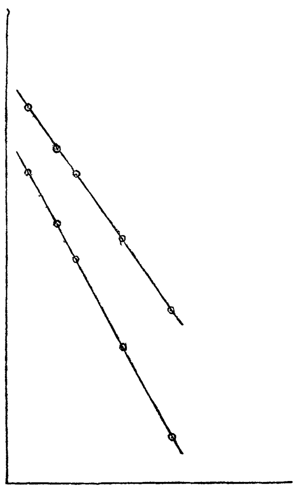
[1] "Alcuni appunti sulla distribuzione del reddito e del patrimonio in Prussia" In Festgaben fur Adolph Wagner

[2] "Zur Methodik der Statistik des Volkseinkommens und Volksvermogens"; "Weitere statistische Untersuchungen uber die Verteilung des Volkseinkommens in Preussen." Both articles in the Zeitschrift des preussischen statistischen Bureaus for 1904.

[3] Weitere statistische Untersuchungen, Anhang.

	Ostpreussen. a	Rheinland a
1892	1 840	1.393
1896	1.893	1.383
1902	1.842	1.359

The excellence of the fit of the curves for incomes above 3,000 marks is illustrated in the accompanying diagram, where the upper line refers to Rheinland and the lower to Ostpreussen. The equations to the lines were computed from the data of 1892 by the Method of Least Squares.



When it is remembered that every important phase of social life is correlated with the form of distribution of wealth, and when it is observed how rapidly the form of distribution is changing with the progress of states from agricultural to industrial stages, one readily admits the degree of importance which Professor Wagner attaches to such inquiries as I have illustrated. "Die Frage, wie sich die Zunahme des Volkseinkommens (und Vermögens) auf die nach der Einkommengrösse gebildeten Zensitengruppen —unsere ökonomischen Stande—verteilt, ist vielleicht der Kernpunkt der sozialen Frage im engeren Sinne des Wortes." [1]

Professor Pareto has himself given a number of illustrations of the use of his method. He has shown that, after the equation to the curve of distribution has been once calculated, it is quite an easy matter to find values

[1] Weitere statistische Untersuchungen, p. 262.

of the median and the percentiles.[1] He has also solved the following problem: Assuming that a proposed progressive tax should affect incomes only above a given rate and that it should never absorb more than a given percentage of an individual's income, what should be the progressive rates of taxation upon the several income-classes in order that the aggregate yield shall be equal to the yield of a given proportional tax?[2]

Pareto's law of income is a purely empirical law, for whose origin Professor Pareto has not offered an explanation. His procedure has been similar to the procedure in physical science in which such laws as those of Boyle, Gay-Lussac, and Avogadro were at first established as purely empirical results. But just as one of the most fertile and characteristic developments of physics since the early work of Clerk-Maxwell has been the rational deduction of these and similar empirical laws from a molecular theory, so likewise has a movement appeared in economics in which an attempt is made to derive from the principles of pure economics the laws established by empirical methods. I offer in illustration of this the French theory of crises.

In his classic work on commercial crises, M. Clément Juglar established the universality of crises, their periodic return and their general resemblance. Defining a crisis as a cessation in the rise of prices (l'arrêt de la hausse des prix) and liquidation as a cessation in the fall of prices (l'arrêt de la baisse des prix), he suggested that the variation of economic factors correlated with prices would afford proper indices of the approach of crises and of the resumption of business after the period of liquidation. He did not succeed in determining the general cause of

[1] "La legge della domanda." Giornale degli Economisti, January, 1895.

[2] La courbe de la répartition de la richesse. Lausanne, 1896. Imprimerie Ch. Viret-Genton.

crises. Indeed, he found considerable satisfaction in the exclusively empirical character of his work: "Sans faire intervenir aucune théorie, aucune hypothèse, l'observation seule des faits a suffi pour degager la loi des crises et de leur periodicité." [1]

The next phase of the theory was developed by M. Pierre Des Essars. Appropriating the suggestion of Juglar that an index of the movement of affairs could be found in the changing values of some factor correlated with general prices, M. Des Essars examined the rapidity of circulation of the deposits at the Bank of France.[2] As a measure of the rapidity of the circulation of deposits, he used the formula $v = \frac{m + m^1}{2s}$, where $v =$ rapidity of circulation, $s =$ the average deposits, $m =$ the aggregate deposits, and $m^1 =$ the aggregate drafts. By means of this formula the values of v for successive years were computed from the data of the Bank of France and the results were compared with M. Juglar's statistics of crises.

According to M. Juglar's account, the periods of crises and of liquidation had appeared in France as follows:—

YEARS OF CRISES AND OF LIQUIDATION IN FRANCE.

1810	crisis	1832	liquidation	1868	liquidation
1811	liquidation	1836	crisis	1873	crisis
1813	crisis	1839	crisis	1877	liquidation
1814	liquidation	1841	liquidation	1882	crisis
1818	crisis	1847	crisis	1886	liquidation
1820	liquidation	1849	liquidation	1891	crisis
1826	crisis	1857	crisis	1892	liquidation
1828	liquidation	1859	liquidation		
1830	crisis	1864	crisis		

[1] Des crises commerciales, edit 1889, p xv

[2] "La vitesse de la circulation de la monnaie." Journal de la Société de Statistique de Paris, 1895, pp 143–149.

In this period of 83 years, crises and periods of liquidation occurred 25 times, and, consequently, the investigation affords ample material for the testing of a theory of the correlation of these phenomena with any other economic factor. After plotting the rapidity of circulation of deposits for 83 years, M. Des Essars discovered that, when allowance is made for the uncertainty as to when a crisis or a period of liquidation actually begins and actually leaves off, then, without a single exception, the curve of circulation reached a maximum at the period of crisis, and reached a minimum at the period of liquidation. M. Des Essars regarded his investigation simply as supplying a barometer for this form of *météorologie sociale.* He made no attempt to isolate the cause of crises.

The third phase in the development of the theory has been sketched by Professor Pareto.[1] It has been generally held by those who have assisted in the construction of economic statics that problems of dynamic economics should be treated by introducing hypotheses as to change, and then making the necessary alteration in the statical theory. Professor Pareto's sketch of the theory of crises exemplifies this doctrine. Unlike M. Juglar and M. Des Essars, he at once makes a hypothesis as to the general cause of crises, and then attempts to show how his dynamic assumption would affect the simultaneous equations to which he had reduced economic statics. According to Professor Pareto, objective conditions determine the form of crises: their cause is the rhythmic change of demand, which is simply one form of a general law of rhythm characteristic of organic life. With this hypothesis as to the cause of the periodicity of crises, he illustrates by a particular case what alterations must be made in his statical theory in order to produce the cyclical movement of

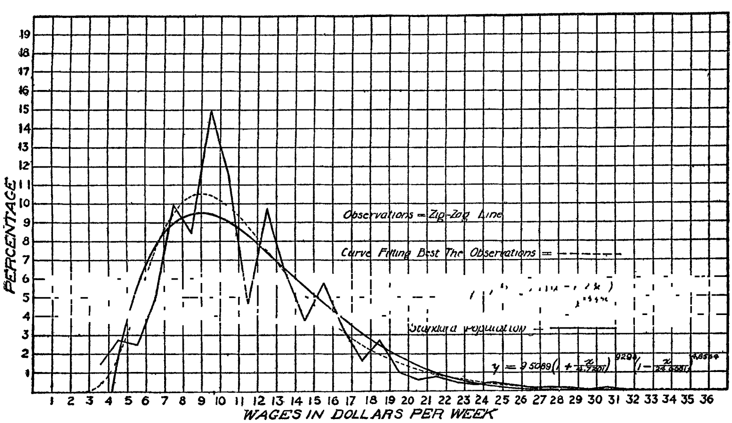
[1] Cours d'économie politique, vol. ii., chap. iv., particularly pp. 282–284.

prices. The step which remains to be taken is to connect Professor Pareto's velocity of the change of demand with M. Des Essars velocity of the circulation of money.

The development of the theory of crises illustrates the attempt to establish deductively results which have at first been reached empirically. The inverse process of establishing statistically results that have been reached in an *a priori* manner may also be illustrated. As an example, I give my own treatment of the efficiency theory of wages.[1]

According to the teachings of pure economics the distribution of wages, where competition is perfect, is determined by the relative efficiency of the laborers. The problem whose solution is attempted is to bring this doctrine to a statistical test.

When the relative frequencies of the rates of wages received by a large group of laborers in varied employments are plotted in the usual manner (illustrated by the accompanying diagram), the zigzag line which describes



[1] Economic Journal, December, 1907 Journal of the Royal Statistical Society, December, 1907.

the distribution ascends very rapidly until it reaches the maximum frequency and then tails off rather slowly, becoming almost parallel to the horizontal line. A smooth curve taking the general direction of the zigzag line may be derived from the statistical data by means of Professor Pearson's method of moments. The dotted curve on the diagram was computed from our census figures for 1900, which are represented by the zigzag line. It is now required to show that, when the efficiency theory of wages is given a numerical form, it will produce a curve approximating this smooth curve.

It is assumed that economic efficiency, like other physical, mental, and moral qualities, is distributed according to the law of error. When any quality is distributed among a group of individuals according to this normal law, it is possible to compute the average difference of the individuals in respect to the quality. Consequently, it is possible to compute the differential efficiency of members of a group. By making a further assumption that in a group of laborers the skilled, organized laborers enjoy a strategic advantage in bargaining which may be estimated in any particular case, a smooth curve may be drawn showing the theoretical distribution of wages. The theoretical curve appears upon the accompanying diagram. The degree of accordance of theory with actual conditions is indicated in the approach of the theoretical curve of distribution to the smooth curve of actual distribution.

To those who are interested in the development of economics as a science there are a few facts which seem to admit of only one interpretation:—

Nearly every one who has dealt with the science as a whole assumes that the statical aspect of our work is relatively complete, and that the investigation of dynamic problems should be entered upon. I have in mind par-

ticularly Professor Clark, Professor Marshall and Professor Pareto.

When we turn to the department of dynamic economics, we find that only two problems are receiving adequate treatment. These problems are the theory of population and the theory of crises. The manner in which the theory of crises is taking on a scientific form has already been described. That the theory of population has in recent years made tremendous strides toward perfection is common knowledge. The correlation with economic factors of changes in population due to changes in birth-rates, marriage-rates and death-rates, has been extensively investigated and exactly described. The change in the quality of the population, together with the causes of the change, is being properly investigated in the work of the school of eugenics. These two departments of dynamic economics, the theory of population and the theory of crises, which alone among dynamic problems are assuming an approximately satisfactory scientific form, and which should therefore serve as models for the development of other departments, owe their present scientific forwardness to the utilization of recent statistical methods in the treatment of questions which had remained in the realm of pure theory.

If it is allowable to base an inference upon the opinion of masters of the science and upon the record of accomplished results, it is not unreasonable to say that at the point which economics has now reached further fecund scientific ideas and abiding practical results are to be found in the development of the *Statistical Complement of Pure Economics*.

HENRY L. MOORE.

COLUMBIA UNIVERSITY.